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MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY.

Communicated by E. B. TITCHENER.

III.

TWO POINTS IN REACTION-TIME EXPERIMENTATION.

By R. WATANABE, PH. M.

We suppose that every psychologist hopes to win something for *psychology* from experiments made upon the simple reaction-time. The simple reaction is the exact type of a voluntary action, and its study should throw light upon the processes involved in this. At present, however, but little has been done towards a psychological interpretation of reaction results, despite the great extent of reaction-literature and the mass of published figures. Indeed, Lange's distinction of the sensorial and muscular forms—which has aroused so much controversy—and Martius' supplementing of it by the introduction of a third (central) form, have constituted almost exclusively the basis upon which psychological discussion has rested.

In the present paper, two questions will be treated: that of the value of subjective (introspective) control of the simple reaction-experiment, and that of the canceling-out of refractory figures. We shall endeavor to deal with each from the psychological point of view.

(1) What is the value of introspective control of the duration of a reaction? Martius ascribes a considerable degree of importance to it, though admitting that introspection is not adequate to the exact estimation of *Störungswerthe*.¹ But the question is not that of whether introspection can determine the relative durations of reactions of 129 σ and 130 σ —to take an extreme instance from Martius²—but rather this: can self-observation place a given experiment under the rubrics, "conditions good, instructions followed," and "conditions bad," or "instructions not followed," with such accuracy, that its verdict suffices at once to admit the experimental result to, or exclude it from the durations employed by the experimenter in averaging? The mean variation of the simple sensorial reaction, *e. g.*, may normally amount, in certain sense departments, to 40 σ . Does self-observation reject times, which, if included in the final calculation, would raise the mean variation above that normal magnitude? We wish again to emphasize the fact that it is for this question a matter of indifference whether

¹Über die muskuläre Reaction und die Aufmerksamkeit. *Phil. Stud.*, VI. pp. 196-7; 204-5; 207; 209-10; 214; 216. Dr. Bliss curiously makes Martius' results entirely negative. *Studies from the Yale Psychological Laboratory*, 1892-3, p. 36.

²P. 204.

we can exactly estimate time-periods of 100-300 σ , or not. We possess now our objective time-norms; we can check our own experiments by reference to those norms; we can pass from the investigation of the "time" of reaction to further investigation. It is, therefore, not enough to state—or to prove—that "the mind is unable to accurately estimate small divisions of time under the most favorable conditions."¹ The "mind" works through the *Reproduktion der Theilelemente des einzelnen Reaktionsvorgangs, soweit er psychischer Natur ist.*² So that—to restate once more—the problem runs: is this reproduction sufficiently accurate to serve as a control of the admission or rejection of the single experimental result in the experimental tables?

Dr. Bliss finds that introspection is "of great value in estimating the general conditions of an experiment and showing the influences which affect the results," though it is "not to be trusted in estimating the results themselves,"³ since our time-estimation is not sufficiently accurate. It is clear, we think, from what has been said above, that the writer has misapprehended the function and manner-of-working of introspection in this particular case. He continues: "Its (the mind's) judgment is particularly liable to be affected by the conditions of the reaction. Its report is what it thinks ought to be, rather than what it actually sees." But this is merely a charging of the "mind" with liability to autosuggestion and *Einstellung*;⁴ and these are "constant errors" in all psychological experimentation, the end of which is the recording of a judgment.⁵ So far, therefore, a clear case against introspection has hardly been made out.⁶

The issue can only be decided by appeal to experimentation. And we believe that, here as elsewhere, strict regard must be had to the sensorial muscular difference. Experiments which were carried out at the Leipzig Institute,⁷ and others which have been made at Cornell,⁸ point, as it seems, convincingly to this conclusion.

In each case the observer, isolated in Lange's way, was given pencil and paper, and required to note down after every experiment his opinion as to the validity of that experiment. The injunction was to the following effect: "Mark all accidental sources of disturbance (stray sounds, entrance of sudden light, 'catching' of the attention by objects in the room, etc.), and state, as explicitly as possible, whether you have reacted as you have been directed (following all instructions as to attention to signal, resting of finger on key, position of head and body, direction of the attention during the experiment, etc.)." Some reagents tended very quickly to confine themselves to such notes as "quick," "slow," but this attempt to estimate the reaction in terms of time was discouraged, it being pointed out that the times were not by any means so important as the qualitative self-analysis, in which introspection (in Martius' sense) furnished a training. Between experiment and experiment there was allowed to elapse a time sufficient for the "settling-down of the mind" to a new essay, unbiased (except for constant influences) by those which had preceded.

¹ *Stud. from the Yale Psych. Lab.*, l. c. The statement is sufficiently daring, in other connections.

² *Phil. Stud.*, VI. 216.

³ *L. c.*

⁴ For which cf. Külpe, *Grundriss*, pp. 208, 470; 44.

⁵ Both in both methods of procedure, the "wissentliches" and the "unwissentliches Verfahren." Cf. *Studies*, etc., 43.

⁶ On p. 44 a position is taken by Dr. Bliss which accords more nearly with our own, but which can hardly be reconciled with that of p. 36.

⁷ *Phil. Stud.*, VIII. 138 ff.

⁸ *This Journal*, Jan., 1894.

We subjoin two specimen tables. In these, *b.* = "bad," *g.* = "good," *s.* = "short," *l.* = "long."

TABLE I.

Dec. 8, 1891. Reagent *Wn.*
Sensorial. Light-stimulus.
Ordinary key. Unit = 1 σ .

1.	499	<i>too l.</i>
2.	810	<i>quite b.</i>
3.	248	<i>g. or s.</i>
4.	342	<i>g.</i>
5.	341	<i>g.</i>
6.	210	<i>too s.</i>
7.	530	<i>too l.</i>
8.	331	<i>g.</i>
9.	251	<i>g. or s.</i>
10.	283	<i>g.</i>
11.	221	<i>s.</i>
12.	348	<i>g.</i>
13.	337	<i>g.</i>
14.	126	<i>quite b.</i>
15.	344	<i>g.</i>
16.	274	<i>too s.</i> [!]
17.	325	<i>g.</i>
18.	367	<i>l. ?</i>
19.	414	<i>l. ?</i>
20.	343	<i>g.</i>
21.	206	<i>s. ?</i>
22.	311	<i>g.</i>
23.	304	<i>g.</i>
24.	290	<i>g.</i>
25.	331	<i>g.</i>
26.	348	<i>g. or l.</i>
27.	278	<i>g. or s.</i>
28.	327	<i>g.</i>
29.	255	<i>g. or s.</i>
30.	292	<i>g.</i>
31.	332	<i>g.</i>
32.	273	<i>g.</i>
33.	322	<i>g.</i>
34.	420	<i>too l.</i>
35.	325	<i>g.</i>

TABLE II.

Dec. 1, 1891. Reagent *Wn.*
Muscular. Light-stimulus.
Ordinary key. Unit = 1 σ .

223	<i>l.</i>
145	<i>g.</i>
190	<i>g.</i>
213	<i>l.</i>
214	<i>s.</i> [!]
165	<i>g.</i>
174	<i>g.</i>
156	<i>g.</i>
289	<i>g.</i> [!]
246	<i>g.</i> [!]
259	? ?
209	<i>g. or s.</i> [!]
142	<i>g.</i>
230	<i>l.</i>
195	<i>g.</i>
194	<i>g.</i>
199	<i>g.</i>
162	<i>g.</i>
195	<i>g.</i>
181	<i>g.</i>
139	<i>g. or l.</i> [!]
112	<i>g.</i>
146	<i>g.</i>
124	<i>g.</i>
202	<i>l.</i>
201	<i>g.</i>
203	<i>g.</i>

[!]

Table I. contains thirty-five sensorial experiments.¹ Eleven of these were rejected by the reagent.² If we average the remaining twenty-four (all those in the record of which the judgment *g.* occurs), we obtain the value (311.8 = reduced) 298 σ , m. v. 27.6 σ ; and practically the same time-value results, if we admit (16) 274, the only case in which the observer's estimate was inaccurate. This was a series taken early in the reagent's course of practice. (His sensorial time finally averaged [reduced] 260 σ .) Introspection is already wholly adequate to the work required of it.³

¹ This, a practice-series, is longer than the experimental-series proper. The best length for the latter seems to be 20-25 experiments. Cf., among others, Dr. Bliss, p. 17.

² *W.* in *Phil. Stud.*, VIII. 138.

³ As we are not dealing here with the qualitative course of the reaction-experiment in detail, we omit from the experimental records all enumeration of the various factors which determined the reagent's judgment. Though valuable in itself, such enumeration would only serve to obscure the present issue by over-weighting the text. We may, however, repeat that the abbreviations in the text do not stand for time-judgments: "l." or "s." is simply an inference (of experimentee or experimenter) from the qualitative judgment or judgments entered in the reagent's protocol.

Table II. shows twenty-seven muscular times, taken from the same reagent, at a somewhat less advanced stage of practice. (Three experiments failed altogether; nothing is said of these in the table.) The time of this observer proved ultimately to be (reduced) 178.6 σ , m. v. 19.8 σ . The result of these practice experiments, according to his own protocol, would be (179.2 = reduced) 189 σ , m. v. 31.3 σ . The length of the reaction-time itself, and the magnitude of the mean variation, might be ascribed to deficiency of practice; and this explanation would hold, in the rough.¹ Looking at the figures in detail, however, we see at once that the introspective criterion is not valid. Let it be remembered that the judgments were not arrived at in terms of the time-value of the whole reaction-process, but in terms of fulfillment or non-fulfillment of experimental directions and conditions. With this standard of comparison (4) and (5) are judged as long and short respectively; (9) and (10) judged as good (these were sensorial reactions, the reagent tending, during his then stage of practice, to slip unconsciously from the muscular form to the sensorial, which was easier for him); (12) judged as short; (21) as long; (25) as long; (26) and (27) as good. Introspection is, therefore, as inadequate as it was in the former instance adequate.

It may be urged that it is unfair to make these practice-experiments the norm of experiment in general. The objection does not hold. As regards sensorial times, the introspective control certainly improves, but can improve only little, with increased practice;² so that Table I. is a stronger witness to its validity in this sphere than a later-taken experimental series would be. As regards muscular times, the introspective control cannot improve with practice; there is nothing present by which it should improve. The observer can only say: right or wrong, in this direction or in that. Now, when practice is complete, one obtains four general types of muscular reaction: (α) correct reactions; (β) (technically) premature reactions; (γ) (technically) false reactions, and (δ) bad reactions, due to inattention, *etc.*, or to disturbance from without. Of these (β) are as a rule regarded by the experimentee as correct; indeed, we shall admit that they *are*, in all probability, qualitatively correct reactions, if we accept Külpe's explanation of them as muscular reactions to memorial representations of the stimulus. (γ) are usually cognized by the reagent as reactions to signal, or to some interferential stimulus. Where this is not the case, they are regarded as correct; and they may be qualitatively correct—answers to the thought, "I have to react muscularly to a sense-impression."³ As to (δ), external disturbances are, of course, controlled without the aid of introspection; while internal disturbances do not, as a rule, occur; so that introspection has but a very restricted field for activity. One frequently obtains an unbroken series of (α). To sum up. During muscular practice, introspection proves untrustworthy. After practice, the sphere in which the coincidence of its verdict with the objective result can be tested is too small for the drawing of a definite conclusion; the reaction tending to become automatically regular, or, when irregular, not admitting the necessary comparison.

It is needless to multiply instances. We shall quote only two other series, chosen at random from a large number.

¹ We do not mean by this to contradict Martius. *Phil. Stud.*, VI. 190.

² Even so, it is crossed by the tendency to automatism in the practised reagent.

³ *Phil. Stud.*, VI. 381.

TABLE III.

Feb. 25, 1893. Reagent *We*.¹
 Sensorial. Sound-stimulus.
 Lip key. Unit = 1 σ .

1.	327 ?
2.	333 ? l.
3.	259 g.
4.	243 g.
5.	257 g.
6.	226 g.
7.	264 g.
8.	416 l.
9.	231 g.
10.	229 g.
11.	270 g.
12.	239 g.
13.	246 g.
14.	222 g.
15.	281 l.

TABLE IV.

Feb. 17, 1893. Reagent *I*.¹
 Muscular. Sound-stimulus.
 Lip key. Unit = 1 σ .

154 s.	[!]
92 g.	?
105 g.	
102 g.	
— g.	[!]
107 g.	or s.
127 g.	
27 g.	[!]
131 g.	
118 g.	or s.
112 g.	

We have not enough experimental material to enable us to form an opinion with regard to the value of introspection in the case of central times.

To summarize: So far as our experience extends, we are convinced that:

1. Introspection affords an adequate control for the simple sensorial reaction;

2. While it is very much less trustworthy in its verdict upon the simple muscular reaction, the conditions of experimentation being unfavorable to its employment.²

(2). Are discrepant figures to be struck out in averaging? The answer to this question is already given, with the answer to the former. (*a*) In the case of sensorial times, it will be best to cancel all those which the reagent regards as untrustworthy; even though there are thereby sacrificed a certain number of results which accord with the established norm. (*β*) With muscular times it is different. All provably false reactions, and with them the obviously premature reactions (Table IV. [5] and [8]), must be put aside. For the rest, arbitrary canceling is, fortunately, rendered unnecessary by the extreme regularity of the reactions obtained from a practised observer. If obdurate results do occur, elimination by making the number of experiments very great seems to us to be the only right mode of procedure.³

¹ This JOURNAL, Jan. 1894.

² By way of a final criticism of Dr. Bliss's results under this head, we would say: (1) That the question, "Are these reactions muscular or sensorial?" (p. 37) raised after the event, can hardly be answered, except schematically, in terms of the figures and their mean variations. It should have been put earlier. And (2) that the results on pp. 33-5 appear to emanate from a too "careful" observer. It is one thing to introspect, another to inspect your introspection.

³ Cf. Dr. Bliss, p. 18.